Scientists' reactions to Marconi's transatlantic radio experiment

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ABSTRACT

It is shown how mathematicians tried to explain that waves could surmount the wall of ocean 160 km high in their travel from England to Newfoundland, and how their explanations were shown to be fallacious by simple physical arguments. Kennelly and Heaviside suggested that the waves were reflected by the upper atmosphere, which they believed to be highly conducting simply because it was at low pressure; the reason for that belief is discussed. Published information about the circuits at the sender and at the receiver in Newfoundland, and in a ship on which the signals were also received, is used in an attempt to deduce the wavelength and the bandwidth of the radiation. Present-day knowledge of the ionosphere is then used to inquire under what conditions the signal could have been received in Newfoundland, and a number of possibilities are listed. All require that the receiver in Newfoundland must have been much more sensitive than that on the ship.

1 INTRODUCTION

In December 1901 Marconi went to Newfoundland, and received radio signals that had originated in a sender at Poldhu in Cornwall. The signals had travelled 3500 km, and, in their travel, had surmounted a wall of ocean 160 km high. Previously radio signals had been received over distances of only a few hundred kilometres, and over hills that were only a few hundred metres high. A little later, in February 1902, he crossed the Atlantic in a ship, the 'Philadelphia', and received signals from the same sender up to a distance of 1120 km by day and 2500 km by night. These new results were surprising in the extreme, and it is interesting to examine how they were received by scientists and technologists. I shall discuss first the reactions of Marconi's contemporaries, and, secondly, the reactions of scientists of the present day.

2 CONTEMPORARY REACTIONS

The most striking thing about the scientists and technologists at the time of the experiment is that they took so little notice. Amongst the engineers, we might expect that the Society of Telegraph Engineers (the Institution of Electrical Engineers as they had been called since 1888) would be deeply interested, but what do we find? In the first Presidential Address after the experiment, delivered in December 1902, Swinburn said:

> Twenty years ago this Institution was chiefly concerned with the development of the telegraph. We can get but few telegraph papers now. This is not because telegraphy is dead; it is because most of its problems are solved, so there is little to discuss . . . It (telegraphy) has passed out of the childhood of technical difficulties and into the manhood of commercial development.[1]

The next Presidential Address was delivered, in November 1903, by Gray, who started by saying:

> The previous President had resigned in time to let a telegraph man be head of the Institution during the recent International Conference in London . . .

Surely he was going to discuss the tremendous importance of the transatlantic experiment for operational telegraphy and for telegraphic science, but instead he said:

> . . . On wireless telegraphy, I have no new developments to report. . . I need only add that there seems no immediate prospect of its seriously competing with the business of the existing telegraph companies.[2]

It is surely remarkable that, at the time of this Address, the Council of the Institution included Fleming (who was technical consultant to Marconi) and Oliver Lodge as Vice-Presidents, and Marconi himself a newly elected member.

If the professional institution most nearly concerned took so little notice, what about the technical Press? At that time the 'Electrician' was the leading journal, and it carried articles on all aspects of power engineering and of telegraphy, and reported meetings on these topics held by scientific and technical organisations. It noticed Marconi's achievement in a series of editorials all of which were aimed at explaining that 'wireless' was in no way competitive with cable telegraphy: nowhere was there any editorial comment on the important scientific problems posed by Marconi.

I fear we must recognise that both the technical Press, and even the professional institution, were much more interested in the possible commercial repercussions of the experiment than they were in its scientific significance. If we study those reactions we must surely come to the conclusion that they were unbecoming to professional engineers. I hope it would not be possible nowadays that there could be similar reactions in a similar situation. Our Institution should be on its guard.

Meanwhile, what about the scientists and theorists, who presumably had no commercial interest? The first reaction had some curious aspects; it came from a mathematician, Macdonald. In 1903 he published a paper on 'The bending of electric waves round a conducting obstacle'[3], and at the end he wrote:

> The results have an immediate application to the question of the propagation of electric waves around the surface of the earth. For propagation through 45 degrees, i.e. over 3000 miles, the amplitude is more than half what it would be in free space.

Soon after that was written two other mathematicians, Rayleigh in England and Poincaré in France, showed that Macdonald had been mistaken. Both replies were of considerable interest for different reasons. Rayleigh wrote:

> Macdonald's results, if they can be accepted, certainly explain Marconi's success; but they appear to me to be open to objection. The first objection that I have to offer is that nothing of this sort is observed in the case of light. The relation of wavelength to diameter of object is about the same in Marconi's phenomenon as when visible light impinges on a sphere one inch in diameter. So far as I am aware no creeping of light into the dark hemisphere through any sensible angle is observed under these conditions even though the sphere is highly polished.

He then proceeds:

> But I shall doubtless be asked whether I have any complaints against the mathematical arguments,

and explains in detail why the mathematics was wrong.[4] The interesting point here is that even a mathematician with the ability of Rayleigh preferred to view the problem in the light of physical common sense rather than through an elaborate piece of mathematics. We should all do well to follow his example.

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Poincaré also noticed the error in Macdonald's work, and a translation of what he wrote in 1903 states that:

> It is important to repeat the calculations taking account of this error, for it is necessary to decide whether the results of Marconi can be explained by existing theories, and result simply from the great sensitivity of the coherer (the detector), or whether they indicate that the waves are reflected by high layers in the atmosphere rendered conducting by their extreme rarification. [5]

Here we see a reference to what is now called the Heaviside layer, and we encounter the interesting suggestion that the upper air would conduct electricity simply because it was at a low pressure.

It is convenient next to see what Heaviside himself said in this context. In the Encyclopaedia Britannica for 1902 he wrote an article entitled 'Telegraphy'; a large part of which is concerned with his own theory of the propagation of waves along telegraph cables. He wrote:

> When a wave sent along wires comes to a sharp bend in the circuit, a new wave is generated at the bend. This combined with the old wave, forms the wave after passing the bend. There is a rapid accommodation of the wave round the wire to the new direction, but if the bending is continuous, instead of abrupt, the accommodation goes on constantly... There is something similar in wireless telegraphy. Seawater, though transparent to light, has quite enough conductivity to make it behave as a conductor for Hertzian waves, and the same is true in a more imperfect manner for the earth. Hence the waves accommodate themselves to the surface of the sea in the same way as waves follow wires. The irregularities make confusion, no doubt, but the waves are pulled round by the curvature of the earth and do not jump off. There is another consideration, there may possibly be a sufficiently conducting layer in the upper air. If so, the waves will, so to speak, catch on to it more or less, then the guidance will be by the sea on one side and by the upper air on the other. [6]

There is no more about the conducting layer, and it seems that Heaviside really thought that the waves could cross the Atlantic simply by guidance over the conducting surface of the sea, but if that is so why do we now call the upper conducting layer the Heaviside layer? I will answer that question presently, but first let us follow up this idea of guidance by the surface of the sea, for that was the idea that most people seemed to find acceptable.

In trying to understand the contemporary point of view it must be remembered that Marconi's success, not only with his transatlantic experiment, but with signalling over shorter distances, had depended on his using an aerial that was connected to earth. * In a mathematical treatment of the problem the source of the waves must therefore be supposed to be on the surface itself. Now the diffraction calculations of Macdonald, Rayleigh and Poincaré were concerned with waves that impinged on a sphere from an outside source, and it was perhaps natural to suppose that guidance by the Earth's surface would be greater if the radiating source were on the surface itself. Although no one investigated the matter in mathematical detail, several people drew pictures purporting to show how the lines of electric force in the waves would 'have their feet tied to the ground', and would be guided round the Earth with little attenuation. Pictures of that kind were used by Fleming (Marconi's technical adviser) and by Marconi himself,[14] but it is perhaps more interesting to note that in 1904 they were also included in a book that Poincaré wrote in collaboration with Vreeland.

It seems as though, in 1903 or 1904, it was generally accepted, on quite inadequate grounds, that an earthed aerial would radiate some kind of surface wave that was guided by the conducting seawater so as to cross the Atlantic ocean with only little attenuation. Let us digress for a moment to see how theorists followed up this idea in later years. Zenneck showed mathematically in 1907[7] that a special kind of surface wave could travel over a conductor, but he did not show how the wave could be launched by an aerial, or how it would behave if the surface were curved. Sommerfeld in 1909[8] was the first to consider the wave that could be launched from a point source situated on a plane conducting surface. He, and others, particularly Weyl in 1919,[9] showed how to discuss the resulting disturbance in terms of a 'surface wave' and a 'space wave'. Finally, in 1937[10] Van der Pol and Bremmer extended the theory to apply to a point source on the surface of a sphere. As might by then have been anticipated, the attenuation of a wave travelling in this way across the Atlantic Ocean was much too great for Marconi's experiment to have been successful.

Let us now return to the neglected suggestion that the waves were guided by a conducting layer in the upper atmosphere. Almost simultaneously with Heaviside's hint of such a possibility, Kennelly in America made a much more specific suggestion. Like Poincaré, he thought that air at a low enough pressure was a good conductor of electricity, and, after having mentioned that waves could be reflected from the conducting surface of the sea, he continued:

> at an elevation of about 80 km (50 miles) a rarefaction exists, which, at ordinary temperatures, accompanies a conductivity to low-frequency alternating currents about 20 times as great as that of ocean water. There is well known evidence that the waves of wireless telegraphy, propagated through the ether and atmosphere over the surface of the ocean, are reflected by that electrically conducting surface. On waves that are transmitted but a few miles, the upper conducting strata of the atmosphere may have but little influence. On waves that are transmitted, however, to distances that are large by comparison with 50 miles, it seems likely that the waves may also find an upper reflecting surface in the conducting rarefied strata of the air. It seems reasonable to infer that electromagnetic disturbances emitted from a wireless sending antenna spread horizontally outwards, and also upwards, until the conducting strata of the atmosphere are encountered, after which the waves will move horizontally outwards in a 50 mile layer between the electrically reflecting surface of the ocean beneath, and an electrically reflecting surface, or successive series of surfaces, in the rarefied air above. If this reasoning is correct, the curvature of the Earth plays no significant part in the phenomena, and beyond a radius of, say, 100 miles from the transmitter, the waves are propagated with uniform attenuation cylindrically, as though in two-dimensional space. [11]

There are two aspects of Kennelly's article that deserve investigation. First, we notice that he suggested, in some detail, that the wave was reflected in the upper atmosphere, whereas Heaviside seemed to believe that it was predominantly guided by the conducting sea. How is it that we now call the upper conducting layer after Heaviside and not after Kennelly? Secondly, why did Kennelly, and others with him, think that air would be a good conductor simply because the pressure was low?

The answer to the first question was given in 1927 by Eccles. He had worked with Marconi just before the famous experiment, and in 1902, at the age of 27, was beginning to be accepted as an expert on 'wireless' topics. In 1912 he wrote a paper in which he gave the name 'Heaviside layer' to that part of the upper atmosphere that reflected radio waves. Later, in 1927 he wrote:

> May I explain why I happened to choose the name 'Heaviside layer' some sixteen years ago? In the spring of 1902 I was writing from time to time on wireless telegraphy in the pages of the 'Electrician', and one day Mr Tremlett Carter, the editor, showed me a letter from Mr Oliver Heaviside which, while discussing other things, asked if the recent success of Mr Marconi in telegraphing fromCornwall to Newfoundland might not be due to the presence of a permanently conducting upper layer in the atmosphere. I believe this letter was shown to various friends of the editor, but I think it was not published. The substance of the suggestion was repeated by Heaviside in his article in the new edition of the Encyclopaedia Britannica which appeared in America and in England in 1902. The suggestion was gradually approved during the years that followed; and about 1910 I used the convenient name 'Heaviside layer' in a paper, to indicate the portion of the atmosphere that functions so usefully for the purposes of wireless telegraphy. [12]

* It is probable that he used this arrangement because he wished to use a relatively long vertical wire which he had to feed with current from the lower end

It appears quite probable that Eccles himself may have played some part in persuading the 'Electrician' not to publish this important letter from Heaviside.

Next let us inquire how it was that, in 1902, Kennelly and Poincaré supposed the air of the upper atmosphere to be a good conductor simply because it was at a low pressure, although by that time it was appreciated that air would conduct only if it were ionised by some external agency. Kennelly quoted from J. J. Thomson's book[13], published in 1896, that 'air at a pressure of 1/100 mm of mercury has a conductivity for alternating currents approximately equal to that of a 25% aqueous solution of sulphuric acid', and it was this conductivity, 20 times greater than that of sea water, that he mentioned in his article. I suppose it is natural that Kennelly, not being in touch with the latest ideas about the ionisation of air, would consult a book, published only six years earlier, by the acknowledged expert, J. J. Thomson. What is more interesting is to ask why, in 1896, J. J. Thomson himself thought that air could have this great conductivity simply because it was at a low enough pressure, and in answering that question we encounter a delightfully simple experiment on which he based his conclusions.

It had been known for some time that, when a luminous discharge was produced by applying a high voltage to electrodes in a tube containing gas at low pressure, most of the applied voltage drop occurred near the electrodes, and there had been many unsuccessful attempts to measure the voltage across the body of the discharge and to deduce the conductivity of the gas. Thomson investigated the matter by placing the low-pressure air, in a glass container, near to a coil in which high-frequency currents were flowing; the varying magnetic field then induced electromotive forces that caused currents to flow in the air. He then realised that he could use a discharge of this kind to measure the conductivity of the air without the complication of inserting electrodes.

He wrote:

> We cannot avail ourselves of any of the ordinary methods to measure the resistance of rarefied gases to these electrodeless discharges; ... One method, which is very easily applied, is based on the way in which plates made of conductors screen off the action of rapidly alternating currents. If a conducting plate be placed between a primary circuit conveying a rapidly alternating current and a secondary coil, the electromagmetic action of the currents induced in the plate will be opposed to that of the currents in the primary, so that the interposition of the plate diminishes the intensity of the currents induced in the secondary. [13]

His measurement was made by interposing the vessel containing the air between the primary and the secondary and then adjusting the air pressure until the e.m.f. in the secondary was reduced to a predetermined threshold. He then replaced the air with acid whose dilution he altered until the secondary e.m.f. again reached the threshold, and he equated the conductivity of this electrolyte to the conductivity of the low-pressure air. It was this conductivity that Kennelly quoted.

In spite of the suggestions of Kennelly and Heaviside, it was usually supposed, in 1904 or 1905, that long-distance radio transmission was possible because of the diffraction phenomena associated with the use of an earthed aerial. In the lecture that Marconi gave when he received the Nobel Prize in 1909, he said:

> With regard to the presumed obstacle of the curvature of the earth, I am of opinion that those who anticipated difficulties in consequence of the shape of our planet had not taken sufficient account of the particular effect of the earth connection to both transmitter and receiver, which earth connection introduced effects of conduction which were generally at that time overlooked... the waves do not propagate in the same manner as free radiation from a classical Hertzian oscillator, but glide along the surface of the earth. [14]

Those who believed in this 'guided-wave' theory then had to find some way of explaining the different ranges of the signal by day and by night, as observed in the 'Philadelphia'. Suggestions were made that 'daylight dissipated the charge on the aerial';[15] that the 'ether drift', being in opposite directions by day and by night, would hinder the waves more by day; [16] and, more reasonably, that ionisation of the atmosphere near the ground, by electron streams or light from the sun, would absorb the waves. [17] It was not until 1912, when Eccles presented a detailed discussion of the 'layer' theory of Kennelly and Heaviside, that absorption of the waves in the upper atmosphere was considered. [18]

3 PRESENT-DAY VIEWS

Engineers and scientists of the present day are unanimous in admiring the bold and imaginative way in which Marconi proceeded in one spectacular step to extend the range of wireless transmissions from the one or two hundred kilometres achieved around the coasts of England to the 3500 km across the Atlantic Ocean. But while they acknowledge his greatness in demonstrating this possibility of long-distance communication, they are puzzled to explain how his experiment could have been successful.

The signals received on the ship were recorded automatically to limiting distances of 1120 km by day and 2500 km by night, and there is no doubt about their reality. The signals received in Newfoundland were heard on telephones under very difficult conditions, and there have even been suggestions that Marconi deceived himself in thinking that he had heard them. Here we use our knowledge of the transmitter, the two receivers and radio-wave propagation to investigate the conditions under which reception in Newfoundland is consistent with the limiting ranges of the ship's reception, and with what is known of the apparatus used.

3.1 THE TWO RECEIVERS

The receiver in Newfoundland made use of a device known as an 'Italian Navy Coherer'. It consists of a drop of mercury placed between two electrodes, and Marconi used it in series with an aerial, a pair of telephones and (probably) a biasing battery. Although he described it as a 'self decohering' coherer, it is probable that it was what we should now call a rectifier, since we know that a succession of sparks at the transmitter produced a corresponding note in the telephones. A sample of this device is now in the Science Museum, and Dr. Grisdale of the Marconi Company has kindly made measurements on it. They show that a 50% modulated radio-frequency oscillation with an amplitude of about 0·05 V is required to give an audible output in telephones.

In Newfoundland the rectifier and telephones were directly in series with an untuned aerial wire of length 130 m supported by a kite. On the ship a tuned aerial, of length 70 m, was coupled to a tuned circuit and to a coherer of the ordinary type which actuated a relay to record the morse dots and dashes.

There were two important differences betwen these two receivers. First, the one in Newfoundland was probably more sensitive, partly because it used a larger aerial, and partly because the signals were received aurally in telephones, whereas those in the ship actuated the recording apparatus. Secondly, the land-based receiver was untuned and would respond to waves of all frequencies, whereas the ship's receiver would respond only to waves within its own reception band.

3.2 Propagation of the waves

We start without making any assumptions about the power or the wavelength of the radiation. Fig. 1 shows, for a given power radiated, how the field strengths at 1120 km and 3500 km by day and 2500 km by night depend on the wave frequency. We first note that because 1120 km by day and 2500 km by night represent maximum distances for reception on the ship, the fields at these two places and times must be roughly equal. This equality implies that the frequency be less than about 0·5 MHz or greater than about 5 MHz. In the range of frequencies greater than 5 MHz the limiting field strength received on the ship would be about ten times as great as that received in Newfoundland at 3500 km by day. In the range between about 0·2 and 0·5 MHz it would be about 100 times as great, and below 0·2 MHz about ten times.

Thus, even if the signal received in Newfoundland were at the limit of audibility, the apparatus there must have been capable of receiving a signal whose field strength was only one tenth or one hundredth of that received on the ship. For reasons given earlier it is possible that it was more sensitive to this extent. But there is another possibility;

Mr G. R. M. Garratt, late of the Science Museum, has made the ingenious suggestion that the waveform of the radiation from the rather unusual spark transmitter might correspond to a very wide power spectrum, and that the untuned receiver in Newfoundland would receive more power than the tuned one on the ship. He has even suggested that in Newfoundland reception might be on the little-attenuated frequencies greater than 5 MHz, whereas the aerial on the ship was tuned to much lower frequencies. To discuss this suggestion in detail let us use what we know of the transmitter to calculate the radiated waveform and the power spectrum.

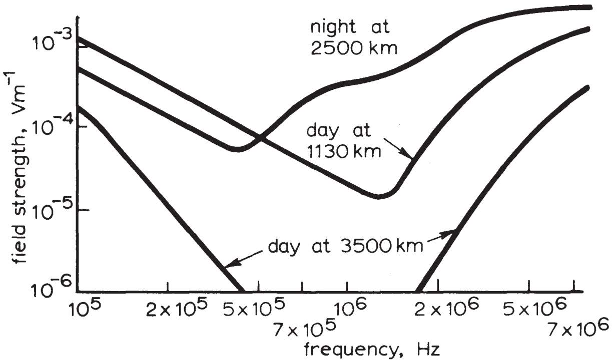

Fig. 1

Field strengths at distances of 1120 km and 3500 km by day, and 2500 km by night, corresponding to a power of 10^5 W radiated, are shown as a function of frequency

3.3 Frequency and waveform of the radiation

The essential circuits of the transmitter are shown in Fig. 2, and enough is known about them to enable us to calculate some important quantities; the detailed calculations are given in Appendix 6. The potential on the capacitor C_1 was increased until the spark gas S broke down; the resulting transient oscillating current i_2 in the aerial then radiated the wave. Circuits 1 and 2 were tuned to the same natural frequency f_0, which, from magnitudes of C_1 and L_1, is found to be about 1 MHz; but we have already decided (Fig. 1) that the radiation could not have been on this frequency.

We must, however, remember that the transmitter made use of two coupled tuned circuits, and if the coupling coefficient is k the spark discharge will excite two different frequencies

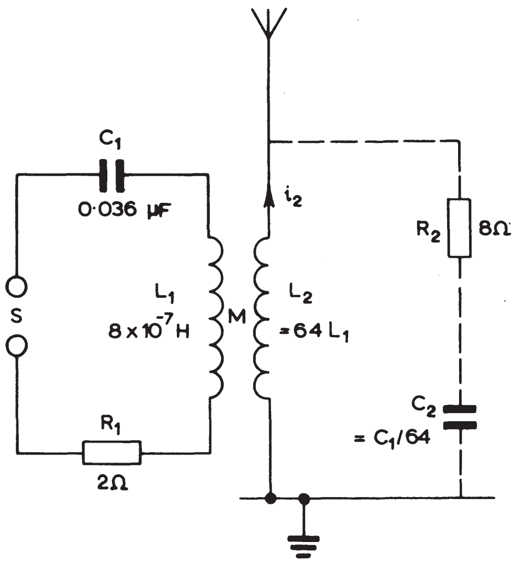

Fig. 2

Essential circuits of the Poldhu transmitter. The circuit constants are given the indicated values for reasons given in the Appendix

$k = M/\sqrt{(L_1 L_2)} = 0{\cdot}95$

equal to $f_0/\sqrt{(1 + k)}$ and $f_0/\sqrt{(1 - k)}$. Now the two coils L_1 and L_2 were constructed in such a way that the coupling was close; for the sake of illustration let us suppose that it had the great (and probably unacceptable) value 0·95. The two natural frequencies would then be 0·7 MHz and 4·5 MHz. The close coupling would also cause the higher frequency to be much more strongly damped than the lower.

The resulting transient current in the aerial circuit has been calculated for me by Dr. Budden and Mr. M. Smith, and is represented in Fig. 3. The two superimposed oscillations, one with frequency 0·7 MHz and slightly damped, and the other with frequency 4·5 MHz and strongly damped, are clearly seen. The whole transient is seen to last for a time of order 5 μs.

The field radiated into free space by the current i_2 in the aerial is proportional to di_2/dt, and the power flux (proportional to the square of this field) has the spectral distribution shown in Fig. 4. The less damped oscillation of smaller frequency corresponds to a comparatively sharp 'spectral line' near 0·7 MHz, whereas the heavily damped greater frequency appears as a very broad spread in the spectrum with a maximum near 3·5 MHz. The heavy damping is responsible for moving this maximum from the frequency (4·5 MHz) it would have if there were no damping. Let us discuss the reception in terms of this power spectrum.

Because the signal received on the ship was tunable, it seems that it must have corresponded to the spectral 'line' at 0·7 MHz, but, as seen in Fig. 1, that frequency is a little too great to explain the known day and night ranges. We must therefore suppose that our knowledge of the circuits is deficient, and that the peak was really at about 0·5 MHz, or perhaps that our propagation curves need revision. The power received by the ship would then correspond to an area under the curve centred on the peak and with a width corresponding to the bandwidth of the receiver.

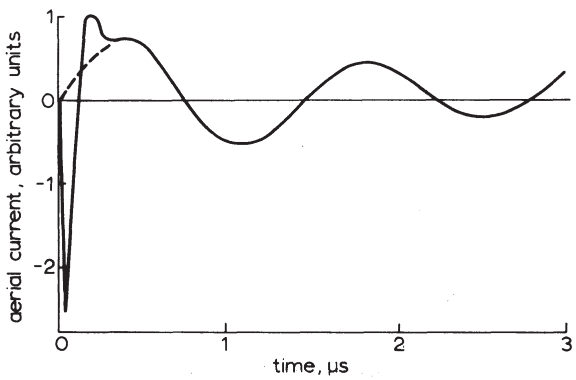

Fig. 3

Waveform of the transient current in the aerial circuit of Fig. 2 when the spark gap S breaks down

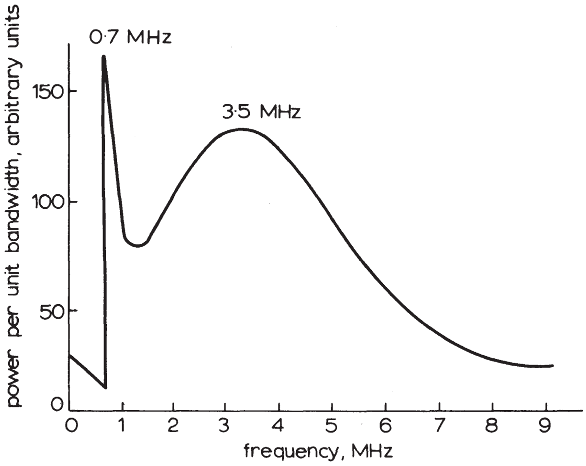

Fig. 4

Power spectrum of the wave radiated by the transient current of Fig. 3

The signal in the untuned receiver in Newfoundland would correspond to the power radiated in those parts of the spectrum that are least absorbed. Reference to Fig. 1 shows that the corresponding frequencies are greater than about 4 MHz and less than about 0·5 MHz: the total power available on these frequencies could, thus, be greater than that available for the ship's receiver.

It thus seems that if we can accept a coupling coefficient as great as 0·95 the receiver in Newfoundland was actuated mainly by waves with frequency greater than 4 MHz, and Mr Garratt's hypothesis is justified. But if a more realistic value of the coupling coefficient is used, the fraction of power on frequencies greater than 4 MHz is not sufficient to explain the difference between reception on the ship and the reception in Newfoundland; and we must fall back on the supposition that the land-based receiver was inherently more sensitive.

3.4 Power radiated and signal received

The foregoing discussion has concentrated on the difference between reception on the ship and the reception in Newfoundland and, although the form of the power spectrum has been calculated, there has been no mention of the absolute magnitude of the power or of the field strength. We now try to estimate the power radiated during one of the transient oscillations, the field strength to which it would give rise to in Newfoundland, and whether that field would be sufficient to actuate the receiver used by Marconi.

The total power in the spectrum can be estimated by supposing that at the start of each train of oscillations the capacitor C_1 is charged to the breakdown potential V_1 of the spark gap so that it contains energy $\frac{1}{2}C_1V_1^2$, which is radiated during the time of 5 μs while the oscillation persists. When V_1 is estimated from a knowledge of the spark gap, the rate of radiation of energy corresponds to a power of about 4×10^7 W.

If the coupling between the two circuits of the transmitter is not too great all this power is radiated near one frequency, it would probably be near 0·5 MHz, or, if the magnitude of the components in the transmitter are different from those we have assumed, it might be 0·2 MHz. Fig. 1 shows what the field strength would be in Newfoundland by day if the power were 10^5 W; for 4×10^7 W radiated the fields must be multiplied by 20, we therefore find about 2×10^{-5} Vm^{-1} at 0·5 MHz and 4×10^{-4} Vm^{-1} at 0·2 MHz.

Next we suppose that the circuits at the transmitter were coupled so closely that the power spectrum has the form shown in Fig. 4, so that, as suggested by Mr Garratt, there is important power on frequencies of 5 MHz and greater. When the total power is 4×10^7 W this part of the spectrum corresponds to a power of about 4×10^6 W, 40 times that for which Fig. 1 is drawn. The corresponding field is then $\sqrt{40}$ (= 6·3) times those in the Figure, and at 5 MHz is about $6{\cdot}3 \times 10^{-4}$ Vm^{-1}.

We now inquire whether these fields, 2×10^{-5} Vm^{-1} (sharply tuned at 0·5 MHz), 4×10^{-4} Vm^{-1} (sharply tuned at 0·2 MHz) and $6{\cdot}3 \times 10^{-4}$ Vm^{-1} (broadly tuned), would be sufficient to actuate the receiver. The untuned aerial had a length of 130 m, so that, if we take its effective height as 65 m, the induced e.m.f. would be, for the three cases, $1{\cdot}3 \times 10^{-3}$, $2{\cdot}6 \times 10^{-2}$ and 6×10^{-2} V.

Let us suppose that all the circuits were completely efficient, that the powers calculated above were all radiated, without any losses in the circuits, and that all the voltage induced in the receiving aerial was applied to the rectifier, and ask whether we should expect to hear the signal. Mr. Grisdale's measurements on the rectifier show that to produce an audible signal a radio-frequency voltage of about 5×10^{-2} V would be required, This voltage is of the same order as those calculated for the broadband radiation and for the radiation of frequency 0·2 MHz. According to our calculation, reception in Newfoundland thus seems just possible with completely efficient circuits, if we assume that either (a) the coupling was very tight so that the coupling coefficient was equal to 0·95, or (b) the coupling was looser and our knowledge of the transmitter circuits is so much in error that the frequency was really 0·2 MHz instead of the calculated 1 MHz.

4 CONCLUSIONS

Our conclusions can be summarised as follows:

(a) From a knowledge only of propagation conditions, reception in Newfoundland is consistent with the observed limiting ranges of reception on the ship only if the land-based receiver was 10 or 100 times more sensitive than the ship's.

(b) From a knowledge of the circuits at the transmitter, together with knowledge of propagation conditions, it is probable that the ship received on a frequency of about 0·5 MHz; the radiated signal might cover a broad band of frequencies.

(c) The calculated power radiated on a frequency of 0·5 MHz would be too small to actuate the receiver in Newfoundland. If, however, the frequency were as low as 0·2 MHz, and if all circuits were 100% efficient, it would be almost sufficient.

(d) If the coupling coefficient of the radio-frequency transformer at the transmitter were as large as 0·95, the power spectrum would contain an appreciable fraction of energy at frequencies greater than 5 MHz.

(e) If the power spectrum had this form, and if all circuits were assumed to be 100% efficient, the calculated power of the transmitter would be almost enough to actuate the detector known to have been used in Newfoundland.

(f) In an earlier paper[23] it was suggested that the coupling coefficient might have been as large as 0·95, but it has since been pointed out to me that in the primary circuit the inductance of the connecting leads would be comparable with the inductance of the single turn on the transformer, so that the coefficient would probably be much smaller than 0·95. In that case only a small fraction of the radiated power would have frequencies greater than 5 MHz.

(g) When we remember that the circuits were probably much less than 100% efficient, that the coupling coefficient was probably less than 0·95, and that the frequency was probably not as low as 0·2 MHz, we must conclude that the receiver sensitivity must have been much greater than that calculated from Mr. Grisdale's measurements on the rectifier.

5 ACKNOWLEDGMENTS

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7 APPENDIX

The circuit constants of Fig. 2 are based chiefly on a description of the transmitter contained in a paper by Entwistle.[19]

$C_1 = 0{\cdot}036\ \mu F$ (from Entwistle)

The inductance L_1 had one turn, consisting of 7-10 wires of diameter 11·82 mm (0·44 in), all in parallel, wound on a square frame of side 45·75 cm (18 in). The equation on p. 53 of Reference 20 then leads to the value $L_1 = 8 \times 10^{-7}$ H.

The coil L_2 consisted of eight turns wound directly on top of L_1, so that $L_2 = 64L_1 = 5 \times 10^{-5}$ H.

Mr Garratt made a copy of L_2, and measured its inductance to be 6×10^{-6} H.

$C_1L_1 = C_2L_2$ (from Entwistle)

The frequency f_0 of either circuit uncoupled to the other is $f_0 = 1/2\pi\sqrt{(L_1C_1)} = 10^6$ Hz.

$R_1 = 2\ \Omega$ (from Fleming, see Reference 2)

$R_2 =$ radiation resistance of aerial with length $l = 50$ m. With frequency 0·6 MHz or wavelength 500 m, then Terman (Reference 20, p. 787) shows $R_2 = 787\ (l/\lambda)^2 = 8\ \Omega$.

The sparking potential V_1 across the gap S, with distance 4 cm between the spheres, is taken fron von Engel (Reference 21, p. 176, Fig. 104) to be about 10^5 V. The energy $\frac{1}{2}C_1V_1^2$ stored in C_1 is then about 180 J. If this is all radiated during a transient oscillation lasting 5 μs, see Fig. 3, the mean power is $3{\cdot}6 \times 10^7$ W.